Technical Report 620

Military Enlistments: What Can We Learn from Geographic Variation?

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U. S. Army

Research Institute for the Behavioral and Social Sciences

February 1984

U. S. ARMY RESEARCH INSTITUTE
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A Field Operating Agency under the Jurisdiction of the
Deputy Chief of Staff for Personnel

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for the Department of the Army

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REPORT DOCUMENTATION PAGE		READ INSTRUCTIONS BEFORE COMPLETING FORM
1. REPORT NUMBER ARI Technical Report 620	2. GOVT ACCESSION NO.	3. RECIPIENT'S CATALOG NUMBER
4. TITLE (and Subtitle) MILITARY ENLISTMENTS: WHAT CAN WE LEARN FROM GEOGRAPHIC VARIATION?		5. TYPE OF REPORT & PERIOD COVERED Final Report Feb 1983 to Sept 1983 6. PERFORMING ORG. REPORT NUMBER
7. AUTHOR(s) Charles Brown		8. CONTRACT OR GRANT NUMBER(s) DAAG29-81-D-0100
9. PERFORMING ORGANIZATION NAME AND ADDRESS University of Maryland National Bureau of Economic Research		10. PROGRAM ELEMENT, PROJECT, TASK AREA & WORK UNIT NUMBERS 2Q263731A792
11. CONTROLLING OFFICE NAME AND ADDRESS U.S. Army Research Institute for the Behavioral and Social Sciences, 5001 Eisenhower Ave., Alexandria, VA 22333-5600		12. REPORT DATE February 1984 13. NUMBER OF PAGES 45
14. MONITORING AGENCY NAME & ADDRESS(if different from Controlling Office) U.S. Army Research Institute for the Behavioral and Social Sciences, 5001 Eisenhower Ave., Alexandria, VA 22333-5600		15. SECURITY CLASS. (of this report) Unclassified 15a. DECLASSIFICATION/DOWNGRADING SCHEDULE

16. DISTRIBUTION STATEMENT (of this Report)

Approved for public release; distribution unlimited

17. DISTRIBUTION STATEMENT (of the abstract entered in Block 20, if different from Report)

18. SUPPLEMENTARY NOTES

19. KEY WORDS (Continue on reverse side if necessary and identify by block number)

Enlistments	Unemployment	Recruiting	Kickers
Economics	Mental category	GI Bill	
Educational benefits	Pay	VEAP (Veterans Educational Assistance Program)	
All-Volunteer Force	Benefits	Ultra-VEAP	

20. ABSTRACT (Continue on reverse side if necessary and identify by block number)

Continued success of the all-volunteer force depends upon the ability of the military services to meet their enlistment goals consistently. This paper measures the effects of economic factors on Army enlistments of nonprior-service high school graduates. The author uses a multiple-regression, pooled cross-section/time-series model to measure Army enlistments by mental cateogry. He concludes that unemployment, pay, and the number of Army recruiters are important variables in the enlistment decision.

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Office, Deputy Chief of Staff for Personnel
Department of the Army

February 1984

Army Project Number
2Q263731A792

Manpower and Personnel

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FOREWORD

The Manpower and Personnel Policy Research Group (MPPRG) of the Army Research Institute performs research in the economics of manpower, personnel, and training issues of particular significance to the U.S. Army. One of these issues is the ability of the Army to attract the desired quantity of recruits to man the force of the 1980s and 1990s.

Since its inception in 1973, the All-Volunteer Force has required the military to compete in the labor market for a declining pool of military-eligible youth. To be competitive, the Army must understand more precisely the effects of certain factors such as unemployment, pay, educational incentives, and recruiting resources on the propensity of individuals to enlist. This report was prepared as part of ARI's continuing support for the Office of the Deputy Chief of Staff for Personnel, including the U.S. Army Recruiting Command.

The research presented in this report helps to quantify several of the factors thought to affect enlistment and contributes to the ongoing theoretical and empirical discussion of military manpower modeling.

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ACKNOWLEDGMENTS

I have benefited from help with data sources and/or comments on a previous draft from George Abrahams, Bernard Bell, Ed Byerly, Michael Cimini, Charles Dale, Curtis Gilroy, Daniel Hamermesh, CPT Philip Knorr, Walter Oi, Thomas Schneider, Marvin Trautwein, and members of the NBER Labor Workshop. Cheryl Hansen provided very careful research assistance. Any remaining inadequacies are my doing. This study was financed by the U.S. Army Research Institute. The judgments expressed are mine, and should not be interpreted as reflecting the position of ARI, the Department of the Army, or the Department of Defense.

MILITARY ENLISTMENTS: WHAT CAN WE LEARN FROM GEOGRAPHIC VARIATION?

EXECUTIVE SUMMARY

Requirement:

The U.S. Army Research Institute conducts research on manpower, personnel, and training issues of particular significance and interest to the U.S. Army. Recently, economic issues in recruitment have become extremely important as the Army faces increased competition from the private sector for a declining pool of qualified manpower. The author has examined some economic variables that affect enlistment decisions and therefore affect the continued success of the All-Volunteer Force.

Procedure:

The author uses a multiple regression, pooled cross-section/time-series model over the 1975-82 period, including pay, unemployment, educational benefits, and recruiting resources as independent variables. This work represents a significant improvement over earlier time-series research because the author has the requisite data across Army recruiting districts to concentrate his empirical research on the period since the inception of the All-Volunteer Force.

Findings:

For high-quality enlistees, defined as those with test scores in the top half of the population or top-scoring individuals who are also high school graduates, a 10-percent increase in military pay raises the supply of enlistees by roughly 10 percent. A 10-percent increase in the unemployment rate (e.g., from 7 to 7.7 percent) increases the number of high-quality enlistees by about 6 percent. Recruiting resources have the expected effects: Army recruiters increase and other services' recruiters reduce Army enlistments. Advertising (both national and local) does not have consistently positive effects. Results are similar for high school graduates, except that the effect of military compensation depends crucially on how it is measured. Estimates of the supply of enlistees of all qualities are weaker still: estimates of compensation effects vary widely, and estimated effects of recruiters and advertising are less plausible. Unemployment effects are smaller than for high-quality recruits but hardly negligible.

A tentative explanation for the weaker results of the latter two groups is that the number of such enlistees is not merely supply-determined but reflects demand constraints as well. Further work is needed to determine how standards for enlistees vary in each recruiting district in response to both national and local fluctuations in recruit supply.

Utilization of findings:

The present work shows that high-quality enlistees are especially affected by compensation and the state of the economy. These findings have implications for military-civilian pay comparability and attractive educational benefits in the continued success of the all-volunteer force.

MILITARY ENLISTMENTS: WHAT CAN WE LEARN FROM GEOGRPAHIC VARIATION?

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I. Introduction

The decision to end the draft in 1973 and return to reliance on voluntary enlistments can be seen as a commitment, an experiment, or (perhaps most accurately) a commitment to an experiment. Given the size of the force considered necessary and a limit to the political willingness to pay for the All-Volunteer Force (AVF), the number of volunteers who could be attracted at politically realistic wages has been a key question.

This supply issue has arisen often in the brief history of the most recent AVF -- first when the end of conscription was being planned, and later when renorming of the entrance test (Armed Forces Vocational Aptitude Battery) showed a decline in the mental abilities of recruits. The more satisfactory recent recruiting experience has been attributed by some to uncommonly high unemployment and by others to the pay increases which occurred at about the same time.

It is difficult to summarize the results of previous econometric work on the supply of enlistees because the supply measure has been defined in so many ways. Some studies analyze total Defense Department enlistments, while others look at enlistments in particular services. Most focus on high-quality recruits, though "high quality" is defined in different ways. Estimates of the elasticity of enlistments with respect to military compensation center a bit below 1.0. Most elasticities with respect to unemployment are much smaller; Morey and McCann's (1983) survey's range of 0.2 to 0.5 is representative. The strongest statement of the unimportance of unemployment is Ash, Udis, and McNown's (1983, p. 147) conclusion that "The evidence on the lack of unemployment effects on accessions is overwhelming."

The purpose of this paper is to examine the supply of enlistees with a different empirical strategy -- pooling cross-section and time-series data from the all-volunteer period. In Section II, the analytic issues raised by previous studies are considered. One conclusion which motivates the remainder of the paper is the difficulty of learning about

the supply of volunteers to a peacetime army from information about enlistments over a period which includes the Vietnam War, the draft, and the post war experience. In Section III, the data for studying determinants of Army enlistments in fiscal years 1976-82 are described. The major results appear in Section IV. While the response of enlistments to compensation is quite complicated, the effect of unemployment on enlistments is unambiguous and larger than typically estimated in previous studies. Conclusions are presented in Section V.

II. Theory and Estimation Issues

The economic theory of voluntary military enlistments was outlined by Oi (1967) and Fisher (1969), and succeeding papers have been (at least implicitly) based on their approach. They introduced a taste variable, d, defined for each individual so that 1+d times the civilian wage W_c is the wage at which the individual is just indifferent between military and civilian employment. Enlistments then depend on the joint distribution of W_c and d, as well as the military wage W_m. To illustrate the main issues involved, Fisher assumed $\ln(W_c)$ and $\ln(1+d)$ were jointly normal. He noted that if $\ln(W_c)$ and $\ln(1+d)$ are independent, and the mean of $\ln(1+d)$ and the variances of $\ln(1+d)$ and $\ln(W_c)$ are constant, enlistments depend on the ratio of W_m to the geometric[1] mean of W_c, or $\ln(W_m)$ minus the mean of $\ln(W_c)$. This proportionality restriction is often convenient in theoretical (Friedman, 1976, pp. 239-40) as well as empirical work on occupational choice. Variations in $\ln(1+d)$ can be handled easily if suitable proxies (e.g., a time trend) are available. However, if $\ln(1+d)$ and $\ln(W_c)$ are correlated, the justification for restricting $\ln(W_m)$ and the mean of $\ln(W_c)$ to have equal and opposite effects on enlistments breaks down.

Fisher noted that observed civilian wages do not include the best opportunity (perhaps zero) open to those unemployed. Thus, the unemployment rate (or some similar

measure) is typically included in enlistment supply functions.

Since Fisher's paper, there have been relatively few theoretical developments in modelling enlistment behavior. Rather, most of the attention has been directed toward correctly estimating the determinants of the supply of volunteers.

One important point which was recognized in Fisher's original paper is that the Armed Forces are neither pure price takers nor pure quantity takers. Rather, they attempt to fill a predetermined number of positions at a predetermined wage, with recruit quality varying to equate supply and demand (DeVany and Saving, 1982). Thus, the number of high-quality enlistees is supply determined, but the total number of enlistees reflects both supply and demand forces (or demand alone if targets are always exactly fulfilled). High-quality enlistees are taken to be those with superior mental-test scores (Altman and Fechter, 1967; Fisher, 1969; Fechter, 1979), high school graduates (Dale and Gilroy, 1983) or high-scoring high school graduates (Cooper, 1977; Grissmer, 1979; Jehn and Shugart, 1979). One recent study of total enlistments cautions that demand constraints may be important (Ash, Udis, and McNown, 1983, pp. 147, 154).

Focusing on high-scoring graduates is probably the best strategy if one's goal is to isolate supply behavior. Studying either high scorers or graduates may reduce but not eliminate the problem of demand-side constraints. Because both test scores and high school graduation are desired attributes, a change in standards for either attribute will change the number of enlistees possessing the other attribute, even if supply conditions and the target number of enlistees is constant. For example, in 1981 the Army restricted enlistments of those with low test scores, leading to a drop in the proportion of enlistees who were high school graduates (Huck, Kusmin, and Shepard, 1982, p. 5) as higher-scoring nongraduates replaced low-scoring graduates. Periodic Congressional pressure to reduce the number of nongraduates may have analogous negative effects on the "supply" of high-scoring enlistees. An unintended experiment in the late 1970s may also have

distorted apparent supply. An error was made in norming the entrance exam, leading to a substantial increase in the proportion of low-scoring enlistees (Office of the Assistant Secretary of Defense, 1982, pp. 17-19) and more enlistments than would have occurred otherwise at the announced wage and intended quality standards. Presumably, the error led to a real increase in the number of high-school graduate enlistees (as some with low "true" scores were mistakenly accepted) and an apparent increase in the number of high-scoring enlistees (unless one uses the corrected score data).

A second issue is the usefulness of pooling data from years in which there was both a war and a draft with data from the peacetime all-volunteer period. The existence of a draft stimulates draft-encouraged volunteers, who enlist "voluntarily" because the prospect of being drafted interferes with civilian employment (Oi, 1967, p. 60), or to secure preferred assignments, or just to avoid being drafted later. Nearly 40 percent of those who volunteered in the period **before** the 1967 Vietnam buildup were draft motivated (Altman and Fechter, 1967, p. 23). On the other hand, some of those who would have eventually volunteered are drafted, reducing the supply of volunteers. There are two strategies for dealing with the effects of the draft. Some studies (e.g., Fisher, 1969; Grissmer, 1979; Ash, Udis, and McNown, 1983) include a variable which measures the probability of being drafted;[2] other studies (e.g., Cooper, 1977) try to determine the number of "true" volunteers during the draft period. Cooper used the enlistment behavior of those with high draft-lottery numbers, who presumably faced little threat of being drafted, to gauge the supply of true volunteers during the lottery phase of the draft.

Each of these strategies has its problems. One difficulty with using a draft-pressure variable is that the probability of induction for "high-quality" individuals (the population being studied) would vary with omitted factors like selective service regulations as well as with the level of overall inductions. The alternative strategy of

inferring true volunteers from enlistments by those with high lottery numbers risks some error because it requires making assumptions about who **perceived** themselves to be draft-safe (Cooper, 1977, p. 194). This is particularly chancy during the early history of the lottery when the eventual probabilities were not well known. Because the draft period was, by design, a period of low military pay and, by accident, a period of low unemployment, errors in holding constant the effect of the draft are likely to bias the remaining coefficients.

Controlling for the effect of the draft, however, is not enough -- supply behavior might be expected to differ in periods of war (or threat of war) and peace, apart from the draft per se, because of "taste" factors such as wishing to be patriotic, or liking or disliking combat. Even the direction of these taste shifts may change as the period of danger wears on. For example, there is evidence of increased voluntary enlistments during the Berlin crisis (Altman and Fechter, 1967, p. 20), but it is not clear that such positive responses continued throughout the prolonged conflict in Vietnam. Fisher (1969), Cooper (1977), Grissmer (1979), and Ash, Udis, and McNown (1983) do not attempt to distinguish observations according to degree of military involvement, leaving the draft-pressure variable and perhaps the time trend to perform this chore as well.

A third estimation issue is lags in the response of enlistments to changes in military pay and civilian alternatives. In general, previous studies have short lags in these responses (one quarter for both relative pay and unemployment in Fisher, 1969; no lags in Cooper, 1977; six quarters for unemployment but none for relative pay in Fechter, 1979; six months for relative pay and two months for unemployment in Grissmer, 1979; one quarter for relative pay and none for unemployment in Ash, Udis, and McNown, 1983).

These lags are either assumed or found to fit better than no lags; longer lags seem not to have been considered. The short lags are consistent with the lag between contract signing and begining service which is sometimes offered as a justification (e.g., Grissmer,

1979, pp. 107-108). It is difficult to say much about the lag between contract signing and beginning of active duty because "enlistment" seems to mean contract signing in some studies (e.g., Devany and Saving, 1982) and beginning of active duty in others (Grissmer, 1979). It is less clear whether they are consistent with their alternative justification -- a desire to avoid reverse causation running from enlistments to the rate of unemployment or the level of civilian pay. In the first place, both unemployment and wages are influenced by the **stock** of military-age individuals in the armed forces; a quarter's fluctuation in the flow of enlistments cannot have a very large effect on this stock (and hence on civilian wages or unemployment). Nor is it clear that a one-quarter lag would be sufficient to avoid any simultaneity that exits if errors in the enlistment equation are serially correlated (Ash, Udis, and McNown's equation for Army enlistments suggests a correlation of about .35 between errors for **semiannual** observations) or if the previously mentioned lag between a decision to join the armed forces and beginning active duty leads to labor force withdrawal during this interval. In any case, the possibility that enlistments are dependent on alternatives expected over a three-year term of enlistment, and that these expectations are based on lagged values of the unemployment rate and level of wages, suggests that the possibility of somewhat longer lags ought to be considered. Of course, the age of the enlistees (typically in their late teens) argues against a very long lag that might be appropriate in other markets.

Dale and Gilroy (1983) follow earlier papers in lagging the unemployment rate, but use the military/civilian compensation ratio with a four-month **lead.** The lead is justified largely on goodness-of-fit grounds. Given that military pay is usually changed only at the start of the new fiscal year, that the change is usually known in advance, and that their dependent variable is the number of contracts signed (rather than the number beginning active duty), a short lead is not implausible.[3]

A final issue which has become important since the expiration of the GI Bill for

new recruits in January 1977 is the appropriate way of incorporating changes in educational benefits into one's measure of military compensation. The GI Bill was replaced by VEAP (Veterans' Educational Assistance Program) which both offered lower benefits and required that the enlistee contribute one dollar for every two dollars the government provided. This was modified in fiscal year 1979 by the introduction of Ultra-VEAP (or the "Kicker"), under which the enlistee received an additional educational benefit for entering hard-to-fill occupations, typically in the combat arms (Military Manpower Task Force, 1982, Chapter V). Unlike the GI Bill and VEAP, the Kicker is available only to Army recruits. While maximum VEAP benefits have remained fixed at $5400 for a three year enlistment, the maximum Kicker grew from $4000 to $12000. Although the matching requirement of VEAP and the limited availability of the Kicker suggest that these dollar values overstate their importance, variations in these benefits are very large relative to the limited variation in other components of military compensation in the post-draft period.

Previous work has had limited success in estimating the effect of these benefits on recruitment. Ash, Udis, and McNown's (1983) military compensation variable ignores the switch from the GI Bill to VEAP and the introduction of modest Kicker payments (their sample ends before the sharp fiscal-year 1982 increase in the Kicker). Dale and Gilroy (1983) treat the GI Bill, VEAP, and the Kicker as separate variables with some success (all are significant in their Army-recruiting equation) but several puzzling results emerge (VEAP is more effective per dollar of benefits than the less restrictive GI Bill for Army, Navy, and Marine Corps recruiting, and neither helps Air Force recruiting).

Thus far, the analysis has concentrated on time-series estimates, which are more common in the literature than are cross-section studies. Viewed in comparison with the time-series approach, the cross-section strategy (e.g., Fisher, 1969; Jehn and Shugart, 1979; Daula, Fagan, and Smith, 1982) offers both advantages and disadvantages. The

main advantage is that one can obtain reasonably-sized samples without including observations which are burdened by draft-era complications. A second feature, which may be both an advantage and a disadvantage, is that there is no[4] variation in military pay across observations. If one is hesitant to accept the relative-pay restriction in the supply function, this is a serious limitation. If one is willing to accept this assumption, at least as an approximation, the lack of pay variation can become an advantage. Without such variation, there is no need to worry about the more complicated forms which such variation can take over time, e.g., the various educational benefits mentioned above.

The major disadvantage of the cross-section approach is the danger that unmeasured taste and ability factors will be correlated with civilian compensation. To cite the simplest example, the historically greater propensity to enlist in the South may be due to lower civilian wage opportunities or to differences in "cultural" attitudes toward the military. To the extent that such attitudes differ only between the South and the North (somehow defined) a dummy variable would solve the problem. But to the extent that both earnings and attitudes differ within regions (New Hampshire and Massachusetts come to mind), the problem is not so easily solved.

Failure to control for differences in entrance test scores across areas can also bias cross-section estimates. Ideally, one might want to study the proportion of high mental ability high school graduates who choose to enlist. In practice, one knows the ratio of high mental ability graduates who enlist to total graduates (or, even worse, total population) in the enlistment-prone age group. Thus, the dependent variable will be affected by differences in the proportion of high school graduates who score well on standardized tests, as well as differences in properly defined supply behavior. One might conjecture that test scores among high school graduates would be positively related to civilian pay; on this conjecture, the estimated effect of civilian pay would be less negative than its true effect. The estimated value could even be positive, though this

does not appear to happen in practice (Fisher, 1969; Jehn and Shugart, 1979; Daula, Fagan, and Smith, 1982.[5]

A related omitted-variable issue is the dispersion of earnings. As Fisher noted, this is usually taken as constant in time-series work. But the variance of earnings does differ geographically, so that its omission might be important in cross-section work.

When one considers the strengths and weaknesses of the time-series and cross-section approaches, pooling cross-section data over the all-volunteer period emerges as an alternative worth considering. The cross-sectional dimension allows us to avoid worrying about holding constant the effects of the draft and the Vietnam War, and gives a good deal of variation in unemployment rates and some variation in civilian earnings. Pooling several cross-sections allows one to introduce state-specific dummy variables to deal with state-specific differences in tastes and ability. It also provides some variation in military compensation, though obviously less than would be available in longer time series.

Pooling several cross-sections and adding state-specific intercepts changes the interpretation of the coefficients from that usually offered in purely cross-sectional work. It is often argued that, if there are lags in responses to the independent variables, cross-section studies estimate long-run impacts. This is because differences across states will be primarily "permanent" rather than "transitory" differences, and responses to these permanent differences will be more or less complete. With individual-specific intercepts, variations around state means for each of the variables over the sample period is what is identifying the coefficients, and so a shorter-run response is being captured. Of course, if one believes that such lags are really important, estimating the pattern of response over time would be desirable.

III. Data

The dependent variables used in this study are ratios of the number of contracts signed by male nonprior-service Army enlistees to the enlistment-age population.[6] Contracts cross-tabulated by high school graduation and mental-test category by state for fiscal years 1976-82 were made available by the Defense Manpower Data Center (DMDC). The mental-test categories have been corrected when necessary by DMDC for the norming error mentioned in Section II. Four types of recruits were analyzed: total, high school graduates, those in the top half of the test-score distribution (mental categories (CAT) I-IIIA) and high school graduate CAT I-IIIA's. For total and high test score contracts, the dependent variable is expressed as the ratio of contracts to population 18-20 years of age; the population data are from the U.S. Census Bureau (1980, 1982, 1983). For high school graduates and high scoring graduates, the dependent variable is the ratio of contracts to the number of high school graduates in the past three years; the latter series is published by state each year by the National Center for Educational Statistics. Because the numerator of the dependent variable is the number of contracts signed in the quarter, rather than the number of enlistees beginning active duty, the lag between decision to enlist and beginning military service mentioned above is not relevant.

Military pay is most often measured by basic military compensation (BMC) which is a weighted average of basic pay, basic allowance for subsistence, basic allowance for quarters, and the federal income tax advantage. The level of BMC for first-year enlistees was obtained from the Army Research Institute, which also provided data on benefits under the various educational benefit programs mentioned above.

An attempt was made to combine basic military compensation and educational benfits into an overall compensation measure. For various reasons -- the fact that such benefits are received in the future, the possibility that the benefits will not be used at

all, and the reluctance of many VEAP participants to contribute the maximum amount -- these benefits are thought to be "worth" less to the recruit than their stated value. Huck, Kusmin, and Shepard (1982) calculated a range of estimates of the value of each type of educational benefit to the recruit. Using the average of the high and low estimate, and taking account of the fact that the Kicker is available to only about half the high-scoring graduates recruited by the Army, the various educational benefits are combined into a single variable. If total compensation equals BMC plus ED (the value of the educational benefits) then

$$\ln(\text{total compensation}) = \ln(BMC) + \ln(1+ED/BMC)$$
$$= \ln(BMC) + ED/BMC$$

Civilian earnings W_c are measured by the quarterly average (by state) of monthly earnings of private workers, based on Unemployment Insurance (UI) records. Almost all private employment is covered, apart from railroad workers, domestics, agricultural workers, and some employees of small nonprofit organizations. The reported earnings are total earnings, not just the portion of earnings subject to UI taxes.

Current-dollar figures such as those mentioned above require deflation for economy-wide changes in prices; the Consumer Price Index was used for this purpose. A further deflation may be necessary to compare data from different states at one point in time.[7] The value of one dollar of military compensation is, arguably, worth the same to a prospective recruit no matter what state he is currently living in, since his ultimate assignment will bear little relationship to his current residence. However, this is not true for civilian alternatives -- a dollar earned in Massachusetts will (apart from occasional tax-evading runs to New Hampshire package stores) be spent in Massachusetts, so its real value depends on the cost of living in Massachusetts. The regional Consumer Price Indices published along with the National CPI are not the appropriate deflator, since the base year takes a value of 100 in each location. Instead,

data from the Bureau of Labor Statistics' (BLS) Urban Family Budget series for a low-income family of 4 (a budget of about $14,000 in 1981) were used. The index created for each state reflected the BLS budget indices for urban and nonurban areas of the region to which the state belongs, weighted by the share of the state's population which resides in urban and nonurban areas.

The unemployment rates used in this study are quarterly averages of monthly unemployment rates tabulated from the Current Population Survey (CPS). These were provided by the BLS, though they emphasize that the monthly CPS tabulations are "official" unemployment rate estimates only for the 10 largest states. Even these unofficial tabulations were not available for about 20 of the smallest states prior to January 1976; the missing values were imputed from regression equations using 1976-82 data, which related the state unemployment rate to the national rate, the state and national employment-population ratio, and a time trend.

Because military compensation is not the only tool available to the Army to influence the supply of volunteers, controlling for other dimensions of the recruitment effort is desirable. Data on the number of Army recruiters were available for the 1976-82 period. Other relevant variables were available for the slightly shorter period from 1976:4 through 1982:2. These included the ratio of Army to Defense Department recruiters and local and national media advertising expenditures. Each of these series except national media advertising vary by state as well as over time. The four variables used in the estimation were REC, the logarithm of the number of Army recruiters divided by the military-age population; DoD, defined similarly for Defense Department (i.e., all four services) recruiters; NADV and LADV, the logarithms of real national and local media advertising expenditures respectively, divided by the military-age population.

The desire to build a quarterly file of data by state led to the use of statewide series on civilian earnings and unemployment, rather than the youth-specific variables

which appear in several other studies. Particularly for forecasting purposes, this is not a very serious loss since forecasted values of young workers' wages and unemployment rates would, apart from trend, be very closely related to all workers' wages and unemployment rates. Moreover, the civilian prospects of high school graduates with above-average abilities may be captured as well by the mean values for all workers as by means for "youth" not stratified by high school graduation or mental ability. Finally, the all-worker variables would be even less subject to reverse causation running from enlistment to civilian wages and earnings than would the youth-specific variables.[8]

IV. Results

Before turning to the regression results, it is useful to obtain an impression of the extent of regional variation over time in the most important variables. Regional patterns of the unemployment rate, civilian earnings, and two measures of recruiting success are shown in Table 1. Each variable is expressed as an index number, with the national average in each fiscal year equal to 100. Thus, in the first panel of Table 1, the unemployment rate in the Northeast was 121.7 percent of the national unemployment rate in fiscal year 1976, and declined to 93.0 percent of the national unemployment rate in fiscal year 1982.

The well-known increase in unemployment in the Midwest appears quite clearly in the table, with the unemployment rate there rising from 89 percent of the national average in 1976 to 114 percent in 1982. Relative unemployment is roughly constant in the South (with the index increasing by 4.5 percentage points), and decreases significantly in the Northeast and West.

Civilian earnings show less regional variation. There is a modest rise in the South, a modest decline in the West, and very gentle U and inverted-U patterns in the Northeast and Midwest.

The regional pattern of the two recruiting measures mirrors the regional unemployment variations. Both measures show a sharp drop in the Northeast, where (relative) unemployment rates were falling, and a striking increase in the Midwest, where unemployment was soaring. Indeed, by either recruiting measure, **the Midwest replaced the South as the dominant supplier of recruits to the Volunteer Army.** Relative recruiting rates declined in the West, along with the relative unemployment rate. The one exception to the otherwise-regular pattern is the South, where modest declines in the recruiting index accompanied modest increases in relative unemployment.

Regional variations in civilian earnings are, as noted above, more modest and it is hard to find a clear relationship between them and changes in recruiting success. Thus, in contrast to unemployment effects (where we have a natural "experiment" which provides enormous geographic differences in patterns over time), the pooled cross-sections seem, at least at this level of aggregation, to provide a less promising basis for estimating relative-earnings effects.

Regression results are presented in Table 2. These regressions are based on 28 quarterly observations (1975:4 through 1982:3) for 50 states and the District of Columbia. The dependent variables are those discussed in section III; each is entered in logarithmic form. Basic military compensation, civilian earnings, and the recruiter/population ratio are also entered logarithmically, so that their coefficients can be interpreted as elasticities. Given the variation in unemployment rates noted above, there seemed reason to hope that the unemployment response could be estimated with considerable precision; hence a quadratic specification was adopted. The unemployment rate is in percent, and coefficients of the quadratic unemployment term have been multiplied by 100. The remaining columns of the table are the coefficient of the time trend (which runs from 1 to 28), the standard error (s.e.) of the equation, and the elasticity of the recruiting measure with respect to the unemployment rate at the

sample-mean unemployment rate of 7 percent (η_u). Not shown are dummy variables for individual states and quarter of the year. All equations were weighted by the military-age population (i.e., number of 18-20 year olds).

The first three lines of Table 2 present three specifications for the most clearly supply-determined measure of recruiting success: the ratio of contracts signed by **CAT I-IIIA high school graduates** to total high school graduates (Dep. Var. = A). Presenting three specifications is a reaction to the difficulty of properly measuring military compensation. The first specification is appropriate if we are confident that our method of combining the various educational benefits into the single variable ED is correct, and that the ratio of total military compensation to civilian compensation is what matters. In this case, the logarithm of the total compensation ratio equals $\ln(BMC/W_c)$ + ED/BMC. The coefficient of BMC/W_c is reasonable and significant, but the coefficient of ED/BMC is **much** larger. Thus, as measured, a dollar of ED is worth much more than (rather than as much as) a dollar of BMC. In thinking about these results, note that the coefficient of BMC gives the response to a change in BMC holding ED/BMC constant -- i.e., the response to a proportionate change in both BMC and ED.

One might still believe that, while our ED variable undervalues educational benefits, it does so for each of the various benefit programs and by about the same proportion. In this case, the first specification is correct if the ratio of total military compensation to civilian pay is what matters, while the second specification does not impose the relative-pay restriction. The estimated coefficients for ln(BMC) and $\ln(W_c)$ are opposite in sign and neither is very different from the .64 value in the constrained equation. Indeed, one cannot reject the constraint at the 5 percent level of significance.

The third specification reflects a more pessimistic view of our ability to measure military compensation, but it assumes that it is the ratio of such compensation to civilian earnings that matters in recruiting. In this specification, a set of fiscal-year dummies

are included to hold constant the effect of changes in military compensation; if it is really relative compensation which matters, the coefficient of $\ln(W_c)$ is an estimate of (minus) the elasticity of recruiting success with respect to relative pay. It is reassuring that one can afford such agnosticism -- the relative compensation elasticity of .97 is not terribly different from the previous estimates.

The estimated effects of the unemployment rate are not at all sensitive to imposing the relative-pay constraint when ED is entered separately; when dummy variables replace the pay variables, the point-of-means elasticity falls from .83 to .65. Given the difficulty of estimating unemployment effects with any precision with time-series data (Ash, Udis, and McNown, 1983), the significance of these estimates is worth underlining.

The effect of recruiters on enlistments is positive and significant in the first two specifications. When fiscal-year dummies are added, however, the estimated effect is nearly zero and not statistically significant.

The second three lines of Table 2 report estimates for the same three specifications, but using the ratio of **CAT I-IIIA contracts** to total military-age population as the measure of recruiting success (Dep. Var. = B). The estimated compensation elasticities are a bit higher, the unemployment elasticities are a bit lower, but the results are generally similar to those in the first three lines. Perhaps the most important qualitative difference is that while using the fiscal-year dummies reduces the estimated impact of recruiters, a non-trivial positive impact remains.

When recruitment success is measured by the ratio of **high school graduate contracts** to total high school graduates (in the last three years), the compensation elasticities are less satisfactory (Dep. Var. = C). Taken at face value, the first two specifications suggest that educational benefits significantly increase recruitment, but increased BMC reduces it. The third specification shows quite clearly, however, that

recruitment success is inversely related to civlian alternatives, and the estimated elasticity (-1.11) is fairly sizeable. The effect of military compensation is just not very reliably estimated in this set of equations. Nor is there much evidence that recruiters have an important effect on the supply of high school graduate enlistees.

Estimated unemployment elasticities, however, remain significant in both a statistical and practical sense. Moreover, they are not very sensitive to the choice of military-pay specification.

The difficulties of estimating the effect of compensation on the ratio of total contracts to population is very similar to that encountered in analyzing the high-school graduate measure (Dep. Var. = D). Neither of the estimates based on explicit measures of military compensation (first two lines of this set) are plausible. The compensation elasticity of 2.16 implied by the last specification has the correct sign and is quite large.

Although the estimated effects of unemployment on total contracts are smaller than for the other dependent variables, they are once again statistically significant and consistent across the three specifications. Given the relatively large percentage changes in unemployment rates that have occurred in the past decade, an elasticity of 1/3 implies non-trivial percentage changes in the number of recruits. For example, at a 10 percent unemployment rate, predicted contracts are 12 percent higher than they would be at the sample-mean rate of 7 percent.

An unavoidable question is why the compensation elasticities are more satisfactory for the first two supply measures than for the last two. While the difficulties of measuring such compensation are certainly important, the rough similarity among the alternative estimates for each of the first two measures suggests that this is an unlikely explanation for why the last two measures do not exhibit such stability. A more plausible explanation is that the number of CAT I-IIIA enlistees is supply determined, but the number of total enlistees is largely demand determined. The simplest story along these

lines -- that changes in standards offset any changes in the supply of enlistees -- would predict that estimated effects would be smaller than real ones, perhaps even zero, but would not lead one to expect wrong-signed "effects." This would seem to require that the total number of recruits demanded be negatively related to the offered wage. While military manpower demands are not usually thought of as having a significant demand elasticity in the short run, such a negative correlation could arise either by chance (recall there are only seven fiscal years of data) or because higher military compensation reduces the demand for **new** recruits by improving retention. If in a period of excess supply recruitment standards are raised **uniformly**, one might expect that those variables with significant geographic variation would determine how the demand-limited total of enlistments would be distributed across the country. Thus, demand constraints might be consistent not only with wrong-signed estimates for military pay, but right-signed estimates of the impacts of civilian pay and unemployment. *While DeVany and Saving* have modelled the way in which discrepancies between the supply of enlistees of the desired quality and the desired number of such enlistees is reconciled at the national level, there seems to be little available research on how demand constraints make themselves felt locally.

Table 3 presents estimates for a slightly shorter sample period (1976:4 through 1982:2), for which the additional measures of recruitment effort were available. For those variables which were included in the regressions in Table 2, the results in Table 3 are broadly similar; where they differ, the differences are more the product of the reduced sample than of the added variables. The major exception is the civilian wage coefficient in equations with fiscal year dummy variables. Here, the estimated coefficients are reduced in absolute value by the additional variables.

For the first three measures of high-quality recruit supply, the recruiter variables tell a generally sensible story. Additional Army recruiters (REC) increase enlistments,

but increases in other services' recruiters (DoD), holding REC constant, reduces them. Increasing both Army and other service recruiters by 10 percent, for example, would increase both REC and DoD by .10, and increase enlistments by .10 times the sum of the coefficients of REC and DoD. Since this sum is positive, proportional increases in all services' recruiters increase Army enlistments. National media advertising sometimes has a positive effect, but this is unstable across specifications. Local media advertising, on the other hand, seems to have no detectable positive effect on recruitment. One interpretation is that such advertising is concentrated on areas where enlistments are below expectations, but the limited available evidence seems inconsistent with this explanation (Morey and McCann, 1983).

The estimates for all enlistees (last three lines of Table 3) seem only to reinforce the conclusion that, however these enlistments are determined across geographic areas, the simple supply model does not capture the process very well.

V. Conclusions

In order to achieve an adequate sample size without including draft-period observations, and to take advantage of large regional differences in the path of unemployment in recent years, various ratios of Army contracts signed to relevant population groups were estimated from quarterly data by state for fiscal years 1976-82. In sharp contrast to several previous papers, unemployment rates had quite strong effects on recruitment success. For various categories of **high quality** recruits, the elasticity of contracts signed with respect to the unemployment rate ranged from .4 to .8. For high quality recruits defined as either **those with scores in the top half of the distribution on the military's entrance test** or **those with high scores and a high school degree**, estimates of the elasticity of contracts with respect to military compensation centered on roughly 1.0. For both **total contracts** and contracts signed by **high school graduates** (regardless of

test score) the compensation elasticity could not be estimated with any confidence. This may be because the number of such enlistments is demand constrained rather than supply determined. There was, however, consistent evidence that the number of contracts was inversely related to alternative (civilian) earnings.

Three quite different directions for future research seem desirable, given the results of this paper. First, research on how recruiting standards respond to local or national shortages or surpluses of recruits meeting a given standard would be very useful. Second, the lag (if any) in the response of enlistments to military and civilian compensation, unemployment, etc. deserves greater attention. Third, the estimates presented here assume that, once state-specific fixed effects are held constant, the error term for successive observations for any state are serially uncorrelated. Recent unpublished research by Gary Solon provides a method for revising the estimates if this assumption is violated. While this type of serial correlation would not bias estimated coefficients, Solon's work suggests that one should be skeptical about the statistical significance of marginally-significant coefficients.

TABLE 1. KEY VARIABLES BY REGION BY FISCAL YEAR

(Index Numbers: National Average In Each Fiscal Year = 100)

Region	1976	1977	1978	1979	1980	1981	1982
				Unemployment Rate			
Northeast	121.7	121.7	116.4	114.5	103.8	98.4	93.0
Midwest	88.6	85.8	88.5	92.4	111.8	113.8	114.4
South	88.5	89.8	93.4	93.6	89.8	92.3	93.0
West	112.3	114.0	109.9	106.1	97.5	96.6	100.6
				Civilian Earnings			
Northeast	101.1	100.0	99.6	100.0	100.9	101.4	101.3
Midwest	105.5	106.7	107.0	106.5	104.9	104.7	104.1
South	95.8	96.0	96.4	96.8	97.7	97.8	98.0
West	98.2	97.5	96.7	96.5	96.4	96.1	96.6
				Contracts/Population			
Northeast	94.2	93.6	85.3	84.5	87.8	84.8	86.9
Midwest	98.4	96.0	87.4	91.6	104.2	111.7	115.3
South	107.7	113.1	127.5	123.7	108.8	108.6	104.3
West	95.1	89.7	85.6	87.1	92.0	85.4	86.0
				High Scoring Graduate Contracts/High School Graduates			
Northeast	101.2	99.5	90.7	90.4	85.2	83.0	81.1
Midwest	90.6	95.5	90.5	98.4	109.9	116.0	114.4
South	105.0	103.3	114.4	106.6	96.0	95.6	99.4
West	106.3	102.8	104.5	104.7	110.2	103.8	102.4

TABLE 2. RECRUITMENT EQUATIONS

Dep. Var.	BMC	ED/BMC	Wc	REC	UR	UR^2	TREND	s.e.	ηU
A	.64	10.2	c	.29	.16	-.28	.018	.2338	.83
	(2.9)	(34.1)		(4.9)	(8.8)	(2.7)	(19.4)		
A	.60	10.2	-.72	.28	.16	-.28	.018	.2339	.83
	(2.5)	(33.9)	(2.6)	(4.6)	(8.8)	(2.7)	(12.8)		
A	d	d	-.97	.02	.10	-.06	d	.2465	.65
			(3.3)	(.3)	(5.1)	(.5)			
B	1.04	8.5	c	.30	.09	-.09	.002	.2048	.57
	(5.4)	(32.5)		(5.8)	(5.9)	(1.0)	(2.7)		
B	.94	8.5	-1.19	.29	.09	-.09	.001	.2048	.57
	(4.4)	(32.3)	(4.9)	(5.3)	(5.9)	(1.0)	(1.1)		
B	d	d	-1.50	.14	.07	-.02	d	.2311	.48
			(5.5)	(2.0)	(3.8)	(.2)			
C	-.61	7.1	c	.09	.10	-.09	.014	.2143	.64
	(3.0)	(26.0)		(1.6)	(6.3)	(1.0)	(15.6)		
C	-1.31	6.9	-.55	-.02	.11	-.09	.007	.2100	.65
	(6.0)	(25.6)	(2.2)	(.3)	(6.5)	(0.9)	(5.2)		
C	d	d	-1.11	.03	.08	-.007	d	.2127	.55
			(4.4)	(.4)	(4.7)	(.07)			
D	-1.59	5.5	c	.02	.02	.15	-.009	.2211	.33
	(7.6)	(19.5)		(.4)	(1.4)	(1.6)	(10.6)		
D	-3.10	5.1	-.95	-.20	.03	.17	-.025	.2004	.35
	(14.9)	(19.8)	(4.0)	(3.8)	(1.7)	(1.9)	(20.6)		
D	d	d	-1.79	.09	.05	.03	d	.2050	.36
			(7.4)	(1.4)	(2.9)	(.3)			

Sample: 51 states, quarterly, from 1975:4 to 1982:3 = 1428 observations

Dependent Variables:

A = High Scoring Graduate Contracts/High School Graduates.
B = High Scoring Contracts/Population.
C = Graduate Contracts/High School Graduates.
D = Contracts/Population.

Notes:

c = Coefficient constrained to equal minus coefficient of RMC.
d = BMC, ED/BMC, and TREND replaced by fiscal-year dummies.
t statistics are in parenthese below coefficients.
Each coefficient of the quadratic unemployment term is multiplied by 100.

TABLE 3. RECRUITMENT EQUATIONS WITH ADDITIONAL RECRUITMENT VARIABLES

Dep. Var.	BMC	ED/BMC	W_c	REC	DoD	NADV	LADV	UR	UR^2	TREND	s.e.	η_U
A	1.02	13.5	c	.84	-.27	-.07	-.06	.12	-.17	.012	.1987	.68
	(4.5)	(31.8)		(8.7)	(3.5)	(3.7)	(3.4)	(7.0)	(1.7)	(10.6)		
A	.87	13.6	-1.41	.84	-.29	-.07	-.05	.12	-.16	.010	.1984	.68
	(3.6)	(31.9)	(4.6)	(8.7)	(3.7)	(4.0)	(3.1)	(6.8)	(1.6)	(6.0)		
A	d	d	.20	.45	-.29	.19	-3.0	.10	-.05	d	.2262	.65
			(.6)	(3.6)	(3.2)	(9.8)	(1.5)	(4.9)	(.4)			
B	.94	9.4	c	.79	-.18	.08	-.07	.07	-.03	-.006	.1571	.44
	(5.2)	(27.9)		(10.3	(2.9)	(5.7)	(5.3)	(4.9)	(.4)	(7.0)		
B	.89	9.4	-1.08	.79	-.19	.08	-.07	.07	-.03	-.007	.1572	.43
	(4.7)	(27.8)	(4.4)	(10.3)	(3.0)	(5.4)	(5.1)	(4.8)	(.4)	(5.5)		
B	d	d	-.04	.53	-.15	.26	-.06	.07	-.02	d	.1935	.48
			(.1)	(5.0)	(1.9)	(15.6)	(3.3)	(4.1)	(.2)			
C	-.69	10.0	c	.42	-.05	.01	-.04	.09	-.03	.006	.2019	.62
	(3.0)	(23.2)		(4.2)	(.7)	(.5)	(2.3)	(5.2)	(.3)	(4.8)		
C	-1.45	10.5	-1.33	.42	-.15	-.03	-.02	.08	.02	-.006	.1933	.57
	(6.2)	(25.2)	(4.5)	(4.4)	(1.9)	(1.4)	(.9)	(4.6)	(.2)	(3.7)		
C	d	d	-.34	.31	-.21	.17	-.02	.08	.005	d	.2059	.58
			(1.1)	(2.7)	(2.4)	(10.0)	(1.0)	(4.4)	(.05)			
D	-2.63	5.3	c	.18	.20	.23	-.08	.04	.09	-.023	.1973	.39
	(11.7)	(12.7)		(1.8)	(2.5)	(12.6)	(4.8)	(2.4)	(.9)	(19.9)		
D	-4.1	6.3	-1.2	.18	.02	.16	-.04	.02	.18	-.044	.1637	.30
	(20.6)	(17.8)	(4.6)	(2.2)	(.3)	(10.5)	(2.6)	(1.1)	(2.2)	(32.8)		
D	d	d	-1.01	.31	-.01	.27	-.07	.06	-.01	d	.1797	.39
			(3.8)	(3.1)	(.1)	(17.5)	(4.2)	(3.5)	(.1)			

Sample: 51 states, quarterly, from 1976:4 to 1982:2 = 1173 observations.

Dependent Variables and Notes: Same as those in Table 2.

FOOTNOTES

1. The distinction between the arithmetic and geometric mean is often ignored in subsequent empirical work.

2. More precisely, Fisher used the ratio of total accessions (volunteers plus draftees) to population rather than the ratio of draftees to population. He argued that with target levels of total accessions given, draft calls depend inversely on the number of volunteers, so that the draft/population ratio used in other studies was endogenous.

3. There is a two-month lag between the announcement of the survey results on private-sector pay on which federal pay increases are based and the beginning of the fiscal year. However, pay increases which military personnel actually receive are not automatically determined by the survey results. Therefore, the lag between the decision on military pay and the start of the fiscal year is less than two months.

4. Daula, Fagan, and Smith (1982), using cross-section data on individuals, allow the allowances and tax advantages of the military pay system to vary with marital status and number of dependents, but this variation must be less than that contributed by variations in civilian earnings.

5. Daula, Fagan, and Smith (1982) attempt to account for tastes and ability in a two-equation model in which civilian earnings are determined by the usual factors and the decision to enlist depends on relative pay and various taste factors. However, they assume that several of the earnings determinants do not directly affect enlistments, but operate only through their effect on civilian earnings. Thus, the decline in enlistment propensity as people age is attributed only to the effect of aging raising experience and, hence, earnings; health problems are constrained to increase the probability of enlisting, since they lower earnings and are not allowed to directly affect enlistments.

6. The data and data sources are described in more detail in the Appendix.

7. The three cross-section studies cited earlier apparently did not deflate their civilian-earnings measure to account for regional variations in the cost of living.

8. Ash, Udis, and McNown (1983) use the young-worker unemployment rate, but opt for an instrumental-variable approach. However, apart from other exogenous variables, their instruments are the overall unemployment rate and the minimum wage; the latter is a relatively weak predictor of youth unemployment in other studies (Brown, Gilroy, and Kohen, 1982).

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DATA APPENDIX

(1) **Contracts** - The number of contracts signed by Army recruits each month from October 1975 to September 1982 were provided by CPT Philip Knorr of the Defense Manpower Data Center. The number of contracts was crosstabulated by state, race, sex, prior service, non-prior service, whether high school graduate, and mental category. To maintain comparability with previous research, non-prior service males were studied in this paper.

(2) **Population** - Data on resident population by state by age are available from the U.S. Census Bureau for 1970 and 1980 based on Census counts, and for intercensal years the Census counts are updated to take into account the estimated effect of births, deaths, and net migration. Data for April 1980 and July 1981-82 were taken from U.S. Census Bureau (1983, Tables 1-3), and for April 1970 and July 1971-78 from U.S. Census Bureau (1980, Tables 2-3). Unpublished intercensal estimates for 1979 and 1980 were provided by Ed Byerly of the Census Bureau.

1980 Census population totals exceeded those predicted by the intercensal updating of 1970 Census population by about 2 percent (U.S. Census Bureau, 1982, Table A-1). Thus, the official **national** estimates of population by age for 1971-79 have updated the earlier estimates by assuming that this error in the intercensal estimates occurred gradually between the Censuses. The analogous procedure was applied to the intercensal estimates **by state** for 1971-79. As a check, these updated intercensal estimates were summed across states and compared with the offical national totals (U.S. Census Bureau, 1982, Table 1). Monthly population estimates were then interpolated from the resulting annual estimates. Values for August-September 1982 are extrapolations based on the 1981 and 1982 values.

The estimates of population by state for non-Census years are made in fairly broad age groups, with boundaries at age 5, 14, 18, 21, 45 and 65.

(3) **High School Graduates** - Numbers of graduates (of both private and public high schools) by state by year are published by the National Center for Educational Statistics. Tom Schneider kindly provided 1980-81 data prior to publication, and 1981-82 values were extrapolated from 1979-80 and 1980-81 values. Since contracts can be signed by those still in school for active duty to begin after graduation, the new graduates were assumed to be available for signing contracts in the second quarter (i.e., April-June) of their graduation year. No allowance was made for mid-year graduation.

(4) **Unemployment Rates** - Unemployment rates from the Current Population Survey (CPS) were provided on tape by the Local Area Unemployment Statistics Program of the Bureau of Labor Statistics. For the 10 largest states, these are offical monthly unemployment rates. For the remaining states they are not, and BLS is concerned that they be taken as CPS tabulations rather than official BLS estimates. The official monthly estimates combine CPS tabulations with other data, primarily establishment survey employment figures and unemployment of those covered by unemployment insurance (Norwood, 1977). However, reliable official estimates are not available prior to 1976, except for the 10 largest states. For the period 1976-82 when both series are available for all states, the unweighted correlation of **quarterly** averages was .85. (A weighted correlation would be higher, since the two series are identical for the 10 largest states). The official rate had a larger standard deviation (3.3 vs 2.9), which is surprising since the introduction of non-CPS information in the official-rate calculation is intended to reduce sampling variation.

These tabulations were available for all states for 1976 and later years, but for only 29 states in 1974-75, 28 in 1973, and 16 in 1972. The missing-data states were the

smallest ones, weighted least heavily in the weighted regressions. To provide estimates for these missing data, equations were estimated separately for each state over the period 1976-82. The logarithm of the unemployment rate was a function of the logarithm of the state and national employment/population ratio, a time trend, and month (seasonal) dummies. The employment data were from the BLS series on nonagricultural employment (establishment survey) and the population data were the series on population age 18 and older (as described above). The estimated coefficients were then used to "backcast" the missing unemployment rates.

To check the accuracy of these imputations, the same method was applied to states which did have CPS unemployment rates for 1972-75, and the fit between these values and the predicted values was evaluated. For the 1972-75 period, the actual mean unemployment rate in the states in question was 6.27, the mean of the predicted values was 6.11, and the correlation between the two was .77. Results for 1975 alone were similar.

(5) **Civilian Earnings** - Quarterly earnings of private workers covered by state unemployment insurance laws by state by two-digit industry code were provided on tape by Bernard Bell of the Bureau of Labor Statistics. In 1981, 90 percent of all civilian employment was covered by the Unemployment Insurance (UI) and Unemployment Compensation for Federal Employees (UCFE) programs, the principal exclusions being railroad workers, most domestic workers, agricultural workers, and some employees of small nonprofit organizations (U.S. Bureau of Labor Statistics, 1982a, p. 33). The state by two-digit industry tape deleted "cells" with less than three reporting establishments, leading to very minor discrepancies from published totals, judging from a comparison with published 1980-81 annual averages (U.S. Bureau of Labor Statistics, 1982b, Table 4).

(6) **Nonagricultural Employment** - Monthly total nonagricultural employment levels by state were taken from the BLS Establishment Survey file.

(7) **Cost of Living** - Indices of the cost of living relative to the national average were taken from the BLS Urban Family Budget series, which provide estimates of the cost of obtaining an appropriate market basket of goods in different geographic areas over time. The market basket varies geographically, reflecting differences such as climate-related allowances for utilities and clothing, but does not change over time. The lowest of the three budgets for a family of four, representing expenditures of about $14,000 in 1981, was used.

Indices were published for 25 identified metropolitan areas throughout the period studied and for the nonmetropolitan parts of the four Census regions. An unweighted average of the indentified metropolitan indices was calculated for each region. Each state's index was defined as the weighted average of metropolitan and nonmetropolitan indices for the state's region, with the weights based on the share of its 1980 population in metropolitan and nonmetropolitan areas. The latter was taken from U.S. Census Bureau (1981, Table 3). Honolulu and Anchorage were assigned to Hawaii and Alaska directly, rather than to the West region.

The Urban Family Budget program was discontinued after 1981. Indices for 1982 were calculated by using changes in the Consumer Price Index by region and size of place.

The indices were originally based on data for "autumn" of each year. We associated autumn with the beginning of the fourth quarter of the calender year, and interpolated other quarters.

(8) **Military Pay and Benefits** - The four quarterly compensation series used were those found in Dale and Gilroy (1983): first-year basic military compensation, maximum

monthly benefit for a GI Bill beneficiary, maximum benefit for a Veterans Educational Assistance Program (VEAP) beneficiary, and maximum value of "kicker" payments offered to Army enlistees entering critical specialties.

These educational benefits are likely to be worth less than their stated dollar value to potential recruits, particularly the post-GI-Bill program. A VEAP participant must contribute to his VEAP account (the government matches that conribution on a 2-for-1 basis), and most participants do not make the maximum contribution. The benefits might never be used, if the individual decides against going to college after the Army or decides to stay in the Army. Finally, if they are received, the benefits must be discounted. The average of the high and low estimates of discounted valuations from Huck, Kusmin and Shephard (1982, Table 4) were used to convert educational benefits to current dollars, recognizing that these benefits are earned by three years of service. To value the GI Bill, VEAP, and the Kicker, the values for "Non Contributory Plan," "Contributory VEAP," and "Contributory VEAP with supplemental payment" minus "Contributory VEAP" were used.

(9) **Recruiters and Advertising Expenditures** - A file containing the number of Army recruiters on production, the ratio of Army to Defense Department recruiters, and local and national media advertising expenditures was provided by Marvin Trautwein of the US Army Recruiting Command. Quarterly data were available for fiscal years 1977-82. The number of Army recruiters was available quarterly for fiscal 1976, and annually for 1974-75.

Except for national media advertising, the data were compiled by recruiting districts which are aggregates of counties and often cross state lines. Data by district were converted to data by state using number of persons 18-20 years old in each county from the 1980 Census.

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